APHIS Response to Peer Review of "An Economic Model for Routine Analysis of the Welfare Effects of Regulatory Changes, (V 3.00 Draft, April 20, 2006)"

General response to reviewer comments:

In general, the reviewers found no fault in the model derivations or the underlying microeconomic theory which the model applies. The reviewers found the model to be adaptable to economic welfare analysis in a variety of situations. They found that the model documentation included derivation of virtually all equations and described how different features of the model could be implemented. Reviewers noted that the model appears to be relatively straightforward to implement. The reviewers do indicate that there may be other approaches available today that are more flexible and complete than the model under discussion, which was developed more than 10 years ago.

While reviewers stated that the documentation was straightforward and easy to read, there were a number of specific comments directed at improving the documentation of the model. These improvements would include clarifying specific points and providing more complete discussions of various parts of the model. These comments are well taken and will be addressed in the next version of the model documentation that is produced.

Some specific comments from the reviewers were related to other possible approaches to modeling the sorts of problems that might be addressed with the framework under consideration. None of these comments invalidate the framework itself, but rather point to other resources in other government agencies and universities that might be brought to bear on these sorts of problems, assuming they can be made available to APHIS analysts. Along these lines, reviewers noted that the current model documentation did not include a thorough review and discussion comparing and contrasting all of these other possible approaches. Such a discussion, which would be very valuable, was beyond the intended scope of the current documentation. But it is a point well taken that such an extensive and exhaustive review of alternative modeling approaches would be of value.

There were comments from the reviewers about how far this modeling framework goes, or does not go, in reducing the workload for analysts working on any specific problem. The reviewers noted the lack of pre-defined parameters and the lack of incorporation of specific trade policy mechanisms. The reviewers are correct that these issues would have to be addressed by the analysts working on a specific issue rather than being pre-specified in the modeling framework. If the development of this modeling framework continues, it may be possible to build in more features to further reduce the workload of analysts who may use this framework.

There were some reviewer comments directed specifically at the Armington approach used in the trade portion of the model. The debate about this approach has been in the economic literature for many years. As pointed out in the review, the Armington approach is used in many other large trade models besides the one under discussion. The

economic literature includes econometric evidence suggesting a poor fit between certain trade data and the Armington assumptions.

The bottom line on this argument about the Armington approach is that if, for a specific analysis, there are data to compare such a fit, then the Armington assumptions themselves are unnecessary for that specific analysis. In this situation, the Armington equations in the model are simply turned off and have no effect on other portions of the model.

In the case of international trade analyses, there are rarely data available to perform such comparisons. And this is why assumptions of some type are often used, such as the Armington assumptions, or other spatial price equilibrium assumptions, as mentioned by one reviewer. Through the manipulation of substitution elasticities, the Armington equations allow, to some extent, spatial price equilibrium assumptions to be approximated. So, the Armington approach provides some flexibility in the choice of assumptions.

And, as one reviewer points out, "If net trade is all that is required for a problem, and information about bilateral trade flows is not relevant, this feature of the modeling framework need not be used, and Armington parameters are not necessary." This is quite correct. The Armington equations are included in the model as an option should the analyst desire to use them. And again, if they are not needed or desired, they are simply turned off.

Overall, the comments from the reviewers were very thorough, and they verified the accuracy of the model's underlying equations and it's consistency with microeconomic theory. Areas of possible improvement identified by the reviewers included providing additional clarifications in the documentation, more complete discussions of certain aspects of the model, perhaps including more mechanisms in the model to reduce the workload associated with any specific analysis, and perhaps the need for an extensive and exhaustive evaluation of this model relative to others that are currently housed in other agencies and institutions.

Responses to specific reviewer comments are indented after each comment.

To: Jim Schaub

Re: APHIS's Economic Model for Routine Analysis of the Welfare Effects of Regulatory Changes v 3.00; April 20, 2005

From: Scott Malcolm and Clare Narrod

Under review is documentation that uses a partial equilibrium welfare analysis to measure the effect of regulatory changes on the economy. Specifically, it uses estimates of supply and demand elasticities and current prices and quantities to measure shifts in supply and demand resulting from a regulatory change. From these shifts, changes in welfare can be computed. Presumably, alternative regulatory options could be evaluated under this framework to assess the impact of each. We have some general comments regarding the presentation, and follow these up with specific concerns.

General comments:

For the most part the documentation is straightforward and easy to read. As part of the purpose of the documentation is to explain what is going on to non-economists it would be useful if the notation was explained in more words; perhaps a table would be useful. Similarly it would be useful if examples were given so that non-economists could see how the model described here would capture the impact of potential policies on various sectors. The document includes examples sometimes, but for the most part it does not. On an editorial note, the equation numeration is confusing (equations 1.1 to 5.4); the first digit of the equation number should refer to the "family" of equations, rather than the second digit. It would have also been useful if the spreadsheet had been included so that we could actually see what was programmed, as this is often where there are discrepancies; thus, it is difficult to see if they actually captured what they say they have.

> APHIS Response: To be addressed in editing the next version of the model documentation.

Though in the introduction it states that it cost-effectiveness model or different control strategies…we are not clear that it does this. It would be useful for them to define what they mean by cost-effectiveness as others may use it differently; it seems to us that they are referring to cost-benefit analysis.

> APHIS Response: The model is equally useful for cost-benefit analysis and cost-effectiveness analysis. As used in this context, cost-effectiveness analysis is a subset of cost-benefit analysis where program or policy benefits are taken as given. To be addressed in editing the next version of the model documentation.

It would be useful if some attention was paid to the subject of sensitivity analysis. Clearly, the accuracy of the model is dependent on the estimates for the various elasticities, prices and quantities. Is the spreadsheet set up to facilitate changes of model parameters?

APHIS Response: The spreadsheet platform inherently allows the model parameters to be easily changed for sensitivity analysis. The addition of an add-in software program, such as APHIS Response: Palisade's @Risk, allows for the use of probability distributions for the various model parameters, within the limits of the computing environment.

There are a number of minor typographical errors and omissions that a careful editing should take care of.

APHIS Response: To be addressed in editing the next version of the documentation.

Specific comments:

Under purpose and potential applications:

We suggest that they alter what is written somewhat. We suggest that paragraph one begins by stating why economic analyses are constructed at the departmental level and relating that to what is required under the OMB guidance as how to do cost-benefit and cost-effective analysis instead of stressing what is of interest by the academic community (Lichtenberg and others page 4 paragraph 2).

APHIS Response: To be addressed in editing the next version of the documentation.

Suggest that paragraph two could then discuss the purpose of the various types of models done in a regulatory context (what is listed at on page 4, paragraph 1). Though much is said here it is not clear what they mean by each…in the written part they discuss "relative cost" or cost-effectiveness of different programs, then they go to list benefit costs and the reason to provide methods of analyzing changes in polices and actions. It is not clear to us what they are refereeing to in terms of cost-effectiveness analysis…Intuitively we would think that cost-effectiveness in the context they are describing…i.e., "cost-effectiveness" of different control measures of VS program diseases they would be evaluating the various measures in terms of cost-effectiveness of risk reduction…here it assumes that the control method is effective and they are only measuring it in terms of impacts on society.

APHIS Response: As mentioned above, the model is equally useful for cost-benefit analysis and cost-effectiveness analysis. As used in this context, cost-effectiveness analysis is a subset of cost-benefit analysis where program or policy benefits are taken as given. The efficacy of alternative control measures can be evaluated with the model, in combination with epidemiological modeling, without necessarily evaluating the costs of the control measures relative to program benefits. To be addressed in editing the next version of the documentation.

Suggest that paragraph three remains as it is discussing the market and equity effects of policy changes or activity captured in the model.

Suggest that paragraph 4 expands on how each of these potential applications may be used in the model. We would actually be interested in knowing if the cost-effectiveness in terms of risk could be captured in a variation to the model to provide policy makers with an understanding of the effectiveness of various policy options they are considering in terms of risk reductions not in terms of economic effects on society.

> APHIS Response: As mentioned above, the efficacy of alternative control measures can be evaluated with the model, but this requires the economic model to be used in combination with epidemiological and risk modeling. The scope of the current document, however, is limited to the technical details of the economic model. This economic model can be combined with various epidemiological and risk models to perform a complete evaluation of the efficacy of control measures. The combined economic, epidemiological, and risk analysis would provide the type of information the reviewers allude to regarding risk reduction.

Under analytical methods applied to the model:

In the discussion of the partial equilibrium welfare model, the authors state that this model expands on methods discussed and applied by Lichtenberg et al, Ebel et al, and Forsythe et al, but the readers have no idea of what these other authors did and how this work expands from previous work, if it does. We assume it expands on what the others did because there were limitations to the models referenced. It would be useful to list these limitations and how this model addresses them.

> APHIS Response: Citations for the Lichtenberg, et al, and Ebel, et al, publications are included in the documentation. But certainly some further discussion of these prior works can be added into the documentation. To be addressed in editing the next version of the documentation.

Page 6 it would be useful if they described how the subgroups Q_k are defined, what the authors mean by "approximations of small changes for any functional form", and what they mean by (K) regions. It would also be useful if they defined their entire notation under market equations … eg) Q_m, a_m, Q_d, b_d, Q_e, and b_e. Also, will it be clear to a user of this model what is meant by a "small change"? Is there any guidance as to the extent of changes that can be accurately modeled using this approach?

> APHIS Response: The subgroups are defined by the user of the model depending on the nature of the specific analysis being performed. Examples of subgroups would include different producer types, such as large versus small beef cattle operations, or subgroups could be producers located in different geographic regions with different production practices. The point being that the model parameters for each subgroup would be different. So, the subgroups in the model allow for this type of differentiation between producers.

The boundaries of what constitutes a small change are arbitrary. The larger the change or shock being modeled becomes, the less robust this assumption is. For a general purpose policy analysis model, where turnaround time is an important component of routine analysis, estimating specific functional forms and including them in a model is generally not practical. Thus the use of a simplifying assumption within this model, with the appropriate caveat to the user, that the larger the shock being modeled, the greater the possibility of this assumption being violated. This in turn suggests the possible need to go into a more in-depth analysis, with its associated time and resource requirements.

Page 7 -9 This is fairly straightforward, however it would be useful if they put words to some of their notation for those unfamiliar with economics could understand. It would be useful if this was related to an example throughout so the reader could see what how it would work. Suggest that the authors renumber their equations, currently it is very confusing.

APHIS Response: To be addressed in editing the next version of the documentation.

Page 10, It is good how they use examples here to convey cross-commodity effects. It would be useful if equations 7-10 were accompanied with verbal explanations, focusing on their intuitive plausibility. Also, explain why the other shifters are set to zero. (Also on page 12). Is this a critical assumption, or just a mathematical convenience.

APHIS Response: To be addressed in editing the next version of the documentation. Setting the shifters to zero is a mathematical convenience.

Page 11, Do the sum of the shares alpha i equal 1?

APHIS Response: Not necessarily. The input may be used in the production of commodities not explicitly specified in the model.

Page 25-26: It is not clear how the discussion of modeling shifts over time fits in with the static model described in the preceding pages.

APHIS Response: Essentially, modeling shifts over time in this framework, such as for a disease epidemic that spans multiple years, involves separate instances of the static model being used for each time period included in the overall analysis. The model parameters (prices, quantities, and elasticities) for each successive time period can be adjusted based on the model output from the prior time period. Therefore, overall, the model is not a true dynamic model, but is rather a series of static models.

Comments on "An Economic Model for Routine Analysis of the Welfare Effects of Regulatory Changes,"USDA, APHIS, September 22, 2005

The model represents a substantial amount of work and appears to be adaptable to welfare
analysis in a variety of situations. Positive features included derivation of virtually all equations and comments on how different features of the model could be implemented. It appears relatively straightforward to implement, although some additional explanation may be needed. At times, it was unclear which variables were exogenous and which were endogenous. Of course, this depends upon the purpose of the model and could conceivably change for different uses of the model. Still, the paper could benefit by discussing this issue more explicitly and by describing in more detail the typical policy issues that the model will be used to evaluate. Specific comments on each section are shown below. A caveat should probably be included stating that this model is for policy analysis, not forecasting, and that some variables that will influence actual prices are not included in the model.

> APHIS Response: To be addressed in editing the next version of the documentation.

Base Model

The base model is clear with domestically produced domestic supply, supply of imports in the domestic market, aggregate domestic demand, and export demand for the domestically produced product, and a model closure equation. Each (inverse) supply and (inverse) demand equation has a shifter, which seems like a straightforward way to handle shocks while allowing other variables that would theoretically enter these equations to remain constant unless affected by the shock. The closure (domestic production of good i + imports of good i - exports of good i = domestic demand for good i) appears logical.

1. I didn't understand the reason for the K domestic supply equations. Are these individual
producers whose individual supplies will be summed to make an aggregate market supply function?

> APHIS Response: Yes.

The introduction states that the model is nonspatial, but the section on market equations states that there are K domestic market regions. Obviously the model will be more parsimonious and easier to implement if there is one equation for the aggregate supply of domestically produced goods. If one equation is inadequate, then the reasons for it should be stated and the additional equations should be explained.

> APHIS Response: The model is not truly spatial in nature in that, for example, the movement of commodities between production regions is not explicitly specified in the model. And, in fact, the K production subgroups do not necessarily have to

represent any geographic regions, though they can, but they can also simply represent different types of producers, such as large versus small beef producers.

One aggregate supply equation is inadequate for representing domestically produced goods because a pest or disease agent may effect different types of producers differently, or may effect producers in different geographic regions differently. Therefore, multiple supply equations were included in the model so that these differences could be accounted for in an analysis of a pest or disease event.

2. I was somewhat concerned that there was only one price variable. In a perfect-substitutes
model like this one with an integrated national market, it is reasonable that the supply of domestically produced goods, import supply, and aggregate domestic demand all have the same own-price variable, but taxes or tariffs could separate the demand price from the supply price. If the domestic market consists of K regions, each region could have a different price. Also, the price in the export demand equation could be different. This approach is adequate if there are no major trade barriers and markets are integrated to the extent that the law of one price holds worldwide. For many applications domestic prices will likely differ from foreign prices or world prices, and consideration should be given to adding more price variables to the appropriate equations.

There is only one domestic price variable, though there are different foreign price variables included in the model. This is discussed in the section of the documentation entitled "Modeling Trade Effects." Import tariffs, for example, could be modeled via the gap between the domestic price and the foreign price in the model. More relevant for the specific types of analyses likely to be performed with this model though, specifying different domestic price and foreign price variables allows for modeling the price gaps that may occur as a result of sanitary and phytosanitary regulations that restrict the movement of commodities between the United States and other countries.

APHIS Response: The reviewer is correct in that domestic regional price differences cannot be explicitly addressed in the model. This was a simplifying assumption, essentially averaging prices across the different producer subtypes, and part of what makes this model a non-spatial model. So, essentially what this model does is allow for modeling only a single aggregate producing region in the United States (in other words, there is no transportation component in the model allowing for domestic commodity movement), but also allows for fine tuning of the aggregate supply shift that may occur as a result of a pest or disease event by disaggregating the shocks from a pest or disease epidemic across the different producer subtypes. It's a compromise between an overly simplistic single commodity partial equilibrium approach, and a far more complex and truly spatial model.

Solving the System

This section was good. The math checked, and the resulting equation 6 appears useful (although if some of the earlier suggestions, such as adding price variables, are taken, the equations will change.)

Modeling cross-commodity effects and the derived demand for inputs

My comments on these sections are combined. My main comment concerns the approach, but first a minor comment is made. The math checked on all equations including no. 10 and no 14, although I wasn't sure why a minus sign appeared in equation 10.2 given that it wasn't in equation 10.1. The setup includes supply and demand shifters that could be interpreted as any variables other than price or quantity (which are already included) that might appear in the supply and demand equations. My main comment is that this shifter setup is not directly and fully exploited and that there may be no need to derive equations 10 and 14 because all relevant information already appears in equation 6.

For example, the demand shifter could be the price of a substitute product and its associated cross-price elasticity, or the supply shifter could be an input cost shock (together with an elasticity). These would reflect exogenous changes to the model. Suppose you have a demand equation that includes the price of a substitute and a cross-price elasticity. There is some exogenous policy change that affects the price of the substitute product; this is equal to dbd in your setup. The cross-price elasticity, actually the cross-price elasticity divided by the own-price elasticity because this is an inverse demand function, is equal to in your setup where the superscript indicates that this is a demand price. It appears that what is likely to be most interesting is the change in price of the target good, which is given by equation 6, with which an associated new equilibrium price and quantity could be derived following the policy shock.
My suggestion is calculating dP directly via equation 6 instead of because it seems that dP in response to some exogenous change is exactly what is desired, not the latter expression. The suggestion is also more straightforward. Once dP is known, other relevant variables can easily be calculated.

Similar amounts of information are exogenous in each approach. Equation 10 has expressions for the own-price and cross-price elasticities and a percentage change in the price of the substitute. The suggestion includes the own-price and cross-price elasticities and an absolute change in the price of the substitute. The absolute change could be converted to a percentage change by multiplying the numerator and denominator by the 1/(price of the substitute), or the entire system could be converted to percentage changes, which could be useful, by taking logs and calculating logarithmic derivatives. The suggested change would permit easy calculation of multiple effects on price by simultaneously including expressions for any exogenous factors that might be nonzero in the numerator of equation 6. The individual shifters could also express more complicated shocks.

APHIS Response: The reviewer is correct. The shifter setup is not directly and fully exploited in the current model. And as the reviewer points out, these shifters could be used to express more complicated shocks than for what the model has currently been used for. However, their inclusion does not detract from the current use of the model and allows for possible future enhancements where they may be more fully exploited.

Trade

The comment in the previous section about the use of equation 6 applies to this section, as well. When reading the base equation, I thought export demand would be a generic export demand for the rest of the world, so I was surprised by comments in the first paragraph page 13 about each country having a rest of the world component to account for trade diversion. If the focus of the model is mainly on the domestic market, just having one foreign component may be adequate (see below). Also, in contrast to the initial setup, a distinct foreign commodity price was specified in the excess demand equation (no. 16), which could be more realistic for many situations.

APHIS Response: A generic export demand for the rest of the world is not sufficient within the context of this model. This model essentially sets up two different countries, one as an importer, and one as an exporter. This allows for modeling trade between these two countries, including the effect of any changes in sanitary or phytosanitary regulations. However, the relationship between the importing country and the rest of the world is clearly different than the relationship between the exporting country and the rest of the world. The exporting country may be more likely to be also exporting to other parts of the world, where the importing country may be more likely to be also importing from other parts of the world.

A limitation of the current model is that it cannot represent bi-directional trade flows. In the real world, trade in a commodity sometimes moves both directions between an importing country and an exporting country for a variety of reasons (local price differences, established business relationships, subtle quality differencs, etc.). Within the current model, trade in a single commodity can only move one direction; either as an import, or an export. So, to represent the different relationship that the importing country may have with the rest of the world versus the relationship that the exporting country may have with the rest of the world, it was necessary to divide the rest of the world into two different components to account for the face that the importing country may also be importing from other foreign countries while the exporting country is exporting to other foreign countries.

Obviously the model will have to be adapted to the needs of its users, but it would fit with the base model better if there were just one foreign entity–the rest of the world. It would simplify the model and eliminate the need to deal with trade diversion as all imports would be directly included in the single import supply function. If multiple

countries or a single country and the rest of the world need to be included, it would be preferable to have separate import supply or export demand functions for each one. Expressions for trade diversion (equations 21.1 and 30.1) and trade displacement (30.2) do not fit in well with the base model. These expressions, attributed to Armington, are based on a view of imports of similar goods from different sources as being imperfect substitutes. Thus, the Armington substitution elasticities would have a counterpart in the cross-price elasticities of a demand equation. The Armington approach is usually based on a two-stage budgeting process and the constant elasticity of substitution functional form, which is different from the perfect-substitutes model with linear supply and demand here. Implementation of this approach would also require specification or calibration of the Armington substitution elasticities. The same issues could be addressed in this model framework by including supply equations from different sources that include prices from all markets, so that supply can be shifted as prices change.

> APHIS Response: The reviewer is correct. It would be preferable to model separate import supply and export demand functions for each country that is part of these rest of the world components. However, the data needs and time and resource requirements to do so are quite substantial. Also, these trade flows to and from these rest of the world components are not of much direct interest in the types of analyses likely to be performed with this model. The Armington approach is much simpler to implement, and, though it employs some very restrictive assumptions, was viewed as a reasonable compromise to at least provide some accounting of this "residual trade" occurring with the rest of the world. And, as mentioned in the response to comments from another reviewer, if the analyst wishes to use an approach other than the Armington approach, the Armington equations within this model can simply be turned off and have no effect on other parts of the model.

Many trade models make the small country assumption when a country's imports of a particular commodity are judged to be too small to influence the world price of that commodity or when its exports are considered too small to influence world price. In these situations, the small country is modeled as facing a perfectly elastic supply of imports and a perfectly elastic demand for exports.

> APHIS Response: The reviewer is correct about the frequent use of the small country assumption in many trade models. However, since the focus of the current model is the United States and it's trade with foreign countries, the small country assumption is likely to be invalid in many of the cases for which this model might be used. The approach taken in the current model is a compromise between totally assuming all of these issues away with the small country assumption versus explicitly modeling import supply and export demand functions for all of the "rest of the world" countries.

While obviously not always appropriate, consideration should be given to applying this assumption where valid because it reduces the number of parameters of the model.

It is common in modeling exercises for parameters to proliferate and to calibrate some parameters within the model. While this approach is practical and sometimes necessary, there is danger when, say, the calibrating equations are based on a single realization of volatile processes.

This is a potential problem for calibration or derivation within the model of the export demand and import supply elasticities. Exports and imports, especially from individual countries, are often highly variable. If reliable econometric estimates are not available, it may be desirable to base the calibration of these parameters on several years of data instead of a single year.

> APHIS Response: Agreed. Though outside the scope of the technical details of the model equations themselves, when the model is actually implemented, it is common to estimate parameters based on data averaged over a multi-year span (such as five years).

Deriving the Surplus Equations

This section was clear. Equation 53 was a good succinct but general purpose statement of the change in producer surplus. Two suggestions for improving readability for non-economists: 1) note that the elasticity (p. 23) is the elasticity for the base case (because the functions are linear, the elasticity is non-constant), 2) note that after a shock that the new equilibrium price and quantity are found by equating supply and demand.

> APHIS Response: To be addressed in editing the next version of the documentation.

Also, because consumer surplus is an exact measure of welfare change only under special circumstances, the use of equivalent or compensating variation should be considered. Usually one can derive an expression for the Hicksian demand and thus equivalent or compensating variation given a Marshallian demand. If it is decided to continue with the consumer surplus measure, it should be stated that in these circumstances that it is believed that consumer surplus represents a good approximation to consumer welfare changes.

> APHIS Response: To be addressed in editing the next version of the documentation.

Modeling Shifts over Time

This section seemed generally ok to me, but it was not clear what "perpetual welfare changes"meant or how they are calculated. Are they changes that might result from an infinite horizon model, or is it an instantaneous jump between the initial state and the final state, or something else? Why couldn't the time effects be compared just by estimating the individual welfare effects for each period and discounting them back to some initial period? Does the model implement these features?

APHIS Response: Perpetual welfare changes are an accumulation of the individual welfare effects for each period, discounting them back to the initial period, and extending this accumulation out to an infinite time horizon. It is provided in the model as an option to accumulating these welfare effects over a specified time horizon. The model can provide results from both approaches. To be addressed in editing.

Frank Fillo, Ph.D.
Policy and Program Development
Animal and Plant Health Inspection Service, USDA

Dear Frank:

I am writing to report my review for APHIS of the manuscript "An Economic Model for Routine Analysis of the Welfare Effects of Regulatory Changes." I will attempt to follow the guidelines laid out in your letter and in attachments to that letter. Those emphasize that my review should consider whether the approach outlined in this document is economically sound, and whether the documentation is complete. My assessment of the soundness of this approach will be guided by the objectives laid out in its introduction and the extent to which this modeling framework reaches those objectives, relative to other approaches common in our profession. My assessment of completeness will also consider the extent to which issues related to the modeling choices made in implementing this framework have been explained.

In conducting this review I compared modeling choices in this document to the approaches taken in a variety of different modeling efforts. This approach is most like that employed in early versions of the SWOPSIM model developed by Vern Roningen at the Economic Research Service, USDA(Roningen, Sullivan and Dixit). Agricultural policy models at places like Iowa State-Missouri (FAPRI), Texas A&M and Tennessee are also relevant, if more complex and so more complete than this framework. Trade policy modeling is now often done utilizing computable general equilibrium (CGE) models such as GTAP at Purdue and the World Bank's Linkage model. Each of these models addresses a purpose similar to the modeling framework in this document, and exhibits features and problems related to trying to design a "routine" analytical framework. Documentation of those models would likely be a source of information for an analyst employing this framework, and would suggest alternative specifications of a model as well.

My overall assessment is that the methodology in this manuscript faithfully implements basic microeconomic theory. But it would not get an analyst very far in carrying out a specific assessment of APHIS policies or interventions. In terms of the goals outlined for economic analysis, it meets relatively few, and leaves the more difficult parts to be assessed outside this framework. The documentation is incomplete in the sense that there is no discussion of the modeling choices made, nor of alternatives that might be preferable in a particular situation. Analysts who would use this framework are likely to confront often such choices when applying this framework.

> APHIS Response: Agreed. The documentation does leave it up to analysts who are performing a specific assessment to evaluate this modeling framework versus others that might be available for their specific problem. It was never the intent to create a requirement for analysts to use this model versus others, only to make this one available as well. A complete discussion and review of all possible economic models that might be used for policy analysis was beyond the intended scope of the current document.

It is also incomplete in that it explains how specific formulae for spreadsheet cells are arrived at, but does not explain how these are put together to form a "multi-market

model" and does not provide insight into the crucial steps of determining just how policies or interventions impact producers, nor how costs of policies are determined. Comparison to existing models used by other government agencies (particularly the Economic Research Service of USDA) suggests there are more modern, more flexible and less restrictive approaches now also implemented in spreadsheets which can more fully accomplish the goals stated here. The review below uses specific concerns to show how I arrived at this overall assessment.

> APHIS Response: Same response as above. A complete discussion and review of all available alternative models was beyond the intended scope of the current document. Many inputs into the current model must be determined exogenously, outside the modeling framework. A complete discussion of the epidemiological portions of an analysis, where this economic model may play a part, is a much broader topic than was attempted in this document. How various pest and disease control measures function in reducing risk is also a much broader topic than was attempted here. The only purpose of the current document was to describe the mechanics of the current model.

The introduction in this manuscript clearly states the purpose and goals of this modeling framework. It is designed "to have and maintain a routine systematic method of analyzing the "relative cost" or "cost-effectiveness" of different strategies or control measures of Veterinary Services program diseases… A key goal for the model is reducing turnaround time for analyses." (Forsythe, page 4) To make such an assessment it indicates there are four components:

(1) The yield or unit cost effects of the changes on affected livestock operations,
(2) How market prices and quantities adjust to these effects,
(3) How consumers and producers are affected by the adjustments, and
(4) Government expenditures required to make the changes. (Forsythe, page 4.)

The framework to accomplish these goals is described as "a multi-market, non-spatial, partial price equilibrium, welfare model." (Forsythe, page 5.)

In my view this framework addresses some but not all of these goals. As described, it appears to be a single market model with calculations of impacts from other markets, imposed as exogenous shifts in the equations of the single market modeled. There may be ways to implement the framework described here as a multi-market model. That would require explaining how the spreadsheet framework and equations handle feedback from the central market under investigation back to the secondary markets, which could in turn mitigate the effects modeled here on the primary market. That is either not done, or not explained. Given the nature of the model described, as analytically deterministic, my guess is that it is not done. The documentation suggests there are several sheets pertaining to several markets, but across sheet linkages are never explained. This is one key aspect in which this documentation is quite incomplete.

APHIS Response: The reviewer is correct. The current model does not include a feedback mechanism from the secondary markets to the central market under investigation. It functions as a standard partial price equilibrium model that distributes the primary impacts unidirectionally to the secondary markets. Including a feedback mechanism would require the development of a far more complex model. The across sheet linkages are described in the sections entitled "Modeling Cross-Commodity Effects" and "Modeling Trade Effects." To be addressed in editing the next version of the documentation.

Given that shocks from veterinary disease programs enter this model as exogenous shifters, and since there are no policy variables in the model, it captures neither item (1) nor item (4) above. The effects of policies on producers directly are assessed outside this framework, and simply imposed as supply shifters on it. Any cost assessments appear to be done in that step as well, and not within this model. No equations to calculate government costs are reported. This model therefore cannot be used to look at alternative policy instruments, as that is all done outside the model.

APHIS Response: The reviewer is correct that these shocks are exogenous to the model and therefore this model must be used in combination with other models, such as an epidemiological model for pest or disease agent spread and accounting models for government budgets. It was never the intent for this model to do all of these things alone, but only to be a component in an overall analysis of this type. To be addressed in editing the next version of the documentation.

What this modeling framework does do is to calculate standard welfare measures and overall market price and quantity changes as a consequence of assumed shifts in regional supply functions. This is also one purpose of similarly designed modeling frameworks, although most alternative models would more explicitly incorporate policy instruments and calculate government costs directly within the model. I was quite surprised at the absence of either domestic or trade policy instruments in the equations reported in this document.

APHIS Response: The purpose of the model is to be able to address changes in sanitary and phytosanitary restrictions to trade. These changes are modeled via the gap between the domestic and foreign price. Explicitly incorporating other domestic or trade policy instruments, which are not the direct focus of analyses likely to be performed with this model, would need to be done exogenously were it desired by an analyst for a specific problem. To be addressed in editing the next version of the documentation.

Most of the space in the current document is devoted to derivations of equations to be used in the modeling framework to calculate equilibrium prices, and then quantities, that follow from the externally determined supply shocks. The methods used are basic intermediate microeconomics, and come largely from the textbook referenced in the document (Hirshleifer). In this aspect the documentation is

quite complete. Full details of the derivations of the basic equations utilized in the spreadsheet are provided. Since these are well known results, it is not hard to see that the equations arrived at are correct derivations of the relationships stated. The model starts with supply and demand functions, written in an inverse form. It examines how elasticties may be incorporated into linearized versions of those relationships. It then shows how equilibrium market prices may be computed from assumed elasticties and exogenous shocks to the original inverse supply or demand functions. Prices may then be substituted back into relationships that describe supply and demand in normal rather than inverted form. The paper then goes on to show how changes in secondary markets or input markets can be imposed on the inverse forms of the supply and demand functions. It presumes that the shocks in those secondary markets will be known in terms of quantity rather than price (which will sometimes but not always be the case). This is somewhat harder using inverse forms, as shifters due to quantity changes in basic behavioral equations must be computed in their price equivalent form.

> APHIS Response: This was the basic intention of the documentation; a description of the mechanics of the model.

Problems with this framework come from the decisions made at the beginning in terms of what it does and how it is implemented, and not from these derivations.

Much of what is done here is unnecessary given recent advances in spreadsheet technology, specifically the capability of solving non-linear simultaneous equation systems. That capability has existed in the mainstream spreadsheet platform, Excel, since the late 1990s and was available earlier in other spreadsheets (e.g. Quattro). This methodology may predate that advance, as the citations to applications at the latest are 1994 (Lichtenberg and others, Ebel and others, and Forsythe and Corso). At that time there was a need to derive analytical solutions to simultaneous equation problems, as is done here, or to impose some external solution method, as was done in some early versions of ERS's SWOPSIM model, for example. With the more modern solution methods, the derivations reported here are unnecessary, the exogenous shocks may be directly imposed on either prices or quantities in the underlying behavioral relationships, and non linear functional forms (such as constant elasticity supply or demand) may be utilized.

> APHIS Response: The reviewer is correct, this model was developed more than ten years ago and has been in routine use since that time. There is no doubt that there have been methodological advancements since this time that may simplify the modeling process. However, this fact does not negate the validity of the approach. Much of the reviewer's discussion that follows presents an alternative approach. As has been previously stated, it was beyond the scope of the current document to provide a complete discussion and review of all available alternative models. It would be valuable to develop such a complete review and discussion all available alternative approaches. But again, this task would be outside the scope of the current document.

While econometricians may debate the merits of direct or inverted supply and demand relationships, since the current model linearizes, moving from one form to another is trivial for the equations utilized here. Consider the following simple re-specification of the problem posed here, which takes advantage of writing these equations in direct rather than inverse form to incorporate cross price, multi-market effects:

Supply:	$Qki = Ski(Pi + \gamma ki , Pj)$	$k = 1,..K$
Imports:	$Qmi = Sm (Pi + Ti)$	
Domestic Demand:	$Qdi = Ddi(Pi + \beta, Pj)$	
Exports:	$Qei = Dei(Pi)$	
Market clearing:	$\Sigma kQki + Qmi = Qd\ i + Qei$	

These are equations (1)-(5) from Forsythe written in a direct rather than inverse form (S and D are the inverse of their functional forms in (1)-(4)), with a cross price term and exogenous shifters introduced)(Pj, γki and β), including the most basic policy instrument (a tariff Ti) and with i denoting the primary market while j denotes the secondary market. This can be directly written as one equation in one unknown (Pi) and exogenous terms:

Excess demand: $Ddi(Pi + \beta, Pj) + Dei(Pi) - \Sigma k\ Ski(Pi + \gamma ki , Pj) - Sm(Pi + Ti) = 0$

The spreadsheet strategy would be to exogenously set in some cells Ti, γki and β (and if comparable to this model, also Pj), to feed those numbers into cells containing the explicit formulae for Dd, De, Sk and Sm. Then with one cell containing Pi – which feeds into those formulae – compute for any given Pi the corresponding excess demand. “Solver”, a built in tool of Excel, may then be used to find Pi so that excess demand equals zero, as required by equation 5 (here and in Forsythe).

The multi-market problem can be solved by reproducing equations (1)-(5) for all markets of interest, and simultaneously solving for Pi and Pj (in this two good case, or all of several Pj in multi good cases). That strategy would capture not only the direct impacts from secondary markets, now addressed on pages 10-12, but also any feedbacks from this primary market to that secondary market. This solution strategy was employed in SWOPSIM since the mid to late 1990s.

Since the stated goals of this paper include the ability to rapidly develop an assessment of a policy issue, its simplicity in terms of relying on basic theory, is a virtue. But a key reason for using a simple model is flexibility, and the current solution strategy prevents that. If an analyst wants to deviate from the functional forms or variable inclusions of this framework, the derivations need to be redone, and/or terms changed in the resulting equations utilized in the spreadsheet. With the use of solver, the analyst can substitute other functional forms or add variables determining supply or demand behavior directly. Quantity shifts in secondary markets, and on these markets are also easily incorporated in the more direct, original statement of the

model. Two issues in modeling choice implemented here, but not discussed, can illustrate this point. Linearization need not apply, and modern demand systems, such as LES or AIDS can be employed.

> APHIS Response: As stated in the response to comments from another reviewer, the boundaries of what constitutes a small change are arbitrary. Given a small change, the choice of functional form has no substantive effect on the outcome of the analysis. The larger the change or shock being modeled becomes, the less robust this assumption is. For a general purpose policy analysis model, where turnaround time is an important component of routine analysis, estimating specific functional forms and including them in a model is generally not practical. However, the analyst is free to do so if deemed necessary for a specific analysis. A simplifying assumption was used within this model, with the appropriate caveat to the user, that the larger the shock being modeled, the greater the possibility of this assumption being violated. If the shock of interest is determined to be too large for the application of the linearization assumption, this would suggest the need to go into a more in-depth analysis, with its associated time and resource requirements.

The author correctly states in the text that linearization is appropriate for small changes in market outcomes. But sometimes issues on which APHIS has influence (e.g. mad cow disease?) may have a large impact on markets. Any model will have difficulty when such large changes occur, but non linear functional forms, such as constant elasticity functions, may be know to better represent market behavior over some broader range of changes. Linearity is a quite strong, and as it turns out unnecessary, assumption. The derivations reported here likely would not be possible for many modeling choices, yet the 'simultaneous equations' or 'solver based' solution strategy could easily incorporate some non-linearities.

> APHIS Response: Each of these functional forms, as discussed by the reviewer, have their own restrictions associated with them. Constant elasticity functions, for example, require an assumption that the elasticities do not vary over the range of the impact under consideration. In the case of the linearity assumption used in the current model, the assumption is that the elasticities do vary over this range, but vary in a linear fashion. It is up to the analyst in any specific analysis to determine whether the assumptions being used are appropriate for the situation at hand.
>
> In the current model, on the supply side, the different production subgroups allow different elasticities so be specified for different types of producers. So, while the linearity assumption is imposed, this assumption is somewhat more relaxed on the supply side of the current modeling framework because different elasticities may be used for each production subgroup. This differentiation in producer subgroups, as discussed in the response to comments from another reviewer, is a key feature and benefit of this modeling framework. This approach, including the linearity assumption built into it, follows along with the

established economic literature previously cited from Lichtenberg, et al, and Ebel, et al.

The best example of the use of non linear functional forms commonplace in policy modeling today is the use of theoretically sound demand systems. The analyst starts with a utility function and derives from maximization of that subject to an income constraint demand curves (for all goods, not just the primary market good). The parameters of the utility function can be chosen (or estimated) so that the basic axioms of demand theory are respected. It is quite easy to choose own and cross price elasticties of demand which may violate those axioms. The derivations of demand equations, and constraints on parameters have been worked out for several utility functions. The simplest of these, the linear expenditure system (LES) allows for non homothetic demand (some income elasticties differ from one), which is a problematic assumption of consumer surplus employed here. Use of consumer surplus remains a standard in policy analysis, but better models have mostly incorporated a demand system, like the LES, or a more complicated version, such as the AIDS model, which does a better job of representing cross price effects. A second advantage of this approach is that theoretically superior welfare measures, such as compensating variation or equivalent variation, may then be employed.

Textbook derivations of these models are available (Deaton and Mullbauer, 1980) comparable to the derivations now taken from Hirshleifer. In spite of its name, LES demand equations are non-linear, and AIDS model equations are on linear, as well. It is not uncommon for Taylor series approximations to be used to linearize these demand equations, but that is not necessary if one need not derive analytical solutions of market equilibrium. A practice in the early version of SWOPSIM, which at the time was employing a solution strategy like the one found here, was to utilize such demand systems to impose constraints on choices of demand parameters to insure theoretical consistency. No discussion of this concern is found in this document, but cross price elasticities are used to compute effects from secondary markets.

APHIS Response: The reviewer is correct about the similarity of the solution strategy in the model under discussion to that of the SWOPSIM model produced by USDA's Economic Research Service. However, one of the problems of imposing such constraints, such as was done with SWOPSIM, particularly when using elasticities that are obtained from multiple outside modeling efforts as well as expert opinion, is that the resulting cross-elasticities are sometimes counter-intuitive and have unexpected and possibly questionable signs (negative when they would be expected to be positive and vice-versa). The current modeling framework does not impose these constraints, nor does it restrict an analyst from imposing these constraints, should they be desired for a specific analysis.

One advantage of utilizing one of the well known demand systems is that more advanced and accurate welfare measures (compensating or equivalent variation) can then be computed, since a utility function is specified. These have been employed even in partial equilibrium settings, by specifying an "other good" to capture rest of the

economy effects. They better represent agricultural products, for which some assumptions behind consumer surplus may be invalid (e.g. homothetic demand).

A more difficult challenging to this modeling effort has been raised in recent work by Paarlberg, Lee and Seitzinger on issues of direct relevance to APHIS concerns. They find that that the type of disease shocks APHIS policy addresses may have altered consumer preferences, shifting demand functions and seriously changing the computation of welfare measures like consumer surplus or equivalent variation. Both of those methods require that the utility function is unchanged before and after the market shock. Paarlberg, Lee and Seitzinger discuss ways of trying to adjust welfare measures. There is no discussion of this important concern in this document.

Incorporation of trade effects is another aspect emphasized in the modeling framework documentation (Forsythe, pages 13-21). But no trade policy instruments are incorporated in any equations. The presumption of the model as written is that the U.S. is a large country exporter and importer in the (primary) market in question, and engages in free trade. There may be cases where that is appropriate, but for lots of goods APHIS could be interested in, some or all of those assumptions may be incorrect. An analyst using this framework would have to seriously alter it to incorporate dairy tariff quotas (TRQs), for example. More problematic is that even more implicit assumptions about international market behavior, and the correct modeling strategy, are invoked without discussion.

> APHIS Response: The reviewer is correct that within this modeling framework, the U.S. is assumed to be a large country exporter and importer. Alternatives to this approach, as described by another reviewer, would be to impose a small country assumption. Problems with the small country assumption are discussed in the response to that reviewer's comments.
>
> It not necessarily assumed within the current framework, however, that the U.S. is engaging in free trade in the commodity in question. This is the reason for incorporating a price gap in the model between the domestic and foreign price. A free trade situation can be modeled within the framework, if desired, by bringing this price gap to zero. Granted, there are no specific mechanisms built into the modeling framework to handle specific trade policy problems, such as tariff-rate quotas, (e.g., there is no stair-stepping explicitly built into the domestic-foreign price gap) and such a problem, if relevant to a specific analysis, would have to be addressed exogenously to this model.
>
> The primary purpose of the current framework is to evaluate changes in sanitary or phytosanitary regulations, as opposed to other trade policy devices. For example, an analyst may have to determine exogenously to this framework, the quantity of a commodity that may be traded given the removal of a particular sanitary barrier and given the other trade policy devices that may be in place for that commodity. Given this information from an external analysis, this quantity of the commodity traded can be brought into the current

framework as an exogenously determined value.

A key a parameter of this model, and of agricultural policy generally, is the net export demand elasticity – ηe. This document and the model derivations utilize the method of Bredahl, Meyers and Collins which requires supply and demand elasticities, price transmission elasticities, and market quantity data for all important trading partners. (the number of trading partners allowed for in this framework is not made clear.) At least two alternatives exist to that method of estimating ηe. It is typical in computable general equilibrium models such as GTAP and Linkage to incorporate explicit trade policy instruments or models of trade policy regimes rather than price transmission elasticties. The successor to SWOPSIM at ERS, Hjort's CPPA model, also incorporated direct policy instruments. In domestic policy models, such as those employed at Texas A&M or Tennessee, direct net export demand functions are estimated. This choice of method is controversial. Direct estimation yields lower net export demand elasticties, and explicit policy instrument modeling yields higher elasticties than are found using the price transmission method. My research suggests that the price transmission model captures rather badly the price stabilization regimes it is intended to represent. In the case of the EU variable levy for example, Brehdal, Meyers and Collins suggest the policy regime should correspond with a price transmission elasticity of zero. Estimation of that price transmission elasticity yields a much higher value, well above 0.5, and more decent dynamic models suggests something in between for annual economic models, with lagged adjustment in prices. This implies that the elasticties an analyst must find to implement this method are problematic (supply and demand elasticties for all trading partners are also not readily available without debate). They depend on a correct dynamic specification of price adjustment, and estimation results are not robust over sample periods. Moreover, the dynamics explained at the end of this paper would miss entirely the lagged price adjustments that characterize foreign markets.

> APHIS Response: The reviewer is correct. Obtaining the elasticities to perform this type of analysis is problematic and data intensive where in many instances the necessary data are not readily available. However, this is not a problem of the modeling framework itself, but a problem that is inherent in the analysis of agricultural trade flows, regardless of the modeling approach taken.

In the end, all this or even my model really require is a net export demand elasticity and/or trade policy instruments. The presentation here should have recognized that there are alternative ways of coming up with that parameter, and should have allowed the analyst the flexibility to choose the method that best captured available data and the circumstances surrounding the role of international trade in the market(s) under investigation. The capability of incorporating basic trade policy instruments for the U.S., such as tariffs or quotas, is highly desirable.

> APHIS Response: Agreed. As discussed above, there is nothing that restricts the analyst from determining traded quantities outside this modeling framework, using any approach desired, and bringing them into the framework as

exogenously determined values. So, this flexibility exists for the analyst for any specific analysis being performed.

Another feature of the trade specification of the model, which is chosen but not discussed, is use of the Armington specification to capture effects of bilateral trade flows. (Discovering these assumptions requires carefully looking at parameter definitions and equation specifications, not reading text.) There are alternatives to the Armington framework that may in some cases be superior choices. In their work on the USDA-IATRC trade embargo study McCalla, Abbott, and Paarlberg found that for the reasonably large market impacts under study, the Armington model characterized trade flows as overly inflexible. The polar extreme assumption, a spatial equilibrium model characterized trade flows as overly flexible – reality lay somewhere between these two modeling choices. The Armington framework was suitable only when trade flows already exist, and only for small market changes. The larger the change, the large should be an Armington substitution elasticity. But modelers have found solution methods for models written as Armington become unstable as those substitution elasticties become larger, so treating this specification choice as a choice of parameter values has not worked well.

This assumption is employed in the large trade models (GTAP and Linkage), but has received a good deal of criticism in recent years, as the problems outlined above appeared in analyses using those frameworks. Most competing modeling frameworks have also resorted to assuming Armington substitution elasticties rather than estimating them, as econometric evidence on their magnitude is weak. Moreover, early econometric evidence (Alston, Carter, Pick and Green) indicated this framework did not fit well agricultural trade data, and its assumptions were routinely violated in practice.

APHIS Response: The Armington approach was originally developed to compensate for a lack of empirical information by imposing some fairly rigid theoretical restrictions. The reviewer is quite correct that empirical studies have often found a poor fit between these restrictions and actual data. If the actual data are available, which is often not the case, then there is no need to use the Armington approach. However, if the actual data are not available, which is common, the Armington approach at least provides a mechanism to work with.

As pointed out by the reviewer, the opposite extreme to the Armington approach, namely the spatial equilibrium approach, which is frequently employed in large trade models, also fails to fit actual data well. If the actual data are available, then approaches that use these data should be employed. When the data are not available, assumptions of some type need to be employed. Within the current model, the Armington approach is provided as an available option. By increasing the substitution elasticities, the Armington approach can be made to mimic the spatial equilibrium approach, however, as the reviewer points out, in some cases the solution may become unstable. All

of these issues can be avoided if the actual data are available and the time and resources are available to develop specific estimates of these behavioral relationships for specific cases. However, it is common for international trade data to be scarce.

If net trade is all that is required for a problem, and information about bilateral trade flows is not relevant, this feature of the modeling framework need not be used, and Armington parameters are not necessary.

APHIS Response: Agreed. For most problems encountered by APHIS analysts, the use of the Armington parameters would not be necessary since they are only relevant if there is interest in explicitly modeling effects in the foreign country. Most APHIS analyses are focused only on domestic impacts. But the Armington parameters do no harm by being available within the model if desired for a specific application.

Since I am a trade economist by training, I have focused on the trade specification of this modeling framework, and its lack of trade policy instruments. (Nearly a third of the document was devoted to the trade specification.) In light of the model's focus on impact on a domestic economy, its lack of inclusion of domestic policy instruments is more troubling. Agricultural policy can be complex, and incorporating provisions of farm bills may not be easily accomplished by setting only price based supply or demand shifters. In any case, no such effects are discussed in this document. Once again, writing the model in a more direct form may make addition of policy variables more straightforward and not require redoing the algebraic derivations described there.

Compared to the competing modeling frameworks, this framework is rather incomplete. In analyzing a particular issue many decisions will need to be taken. Markets will need to be defined and an appropriate level of disaggregation chosen. Prices, grades and standards will need to be defined so that data can be collected to implement an actual model. In the other frameworks, most of those decisions have already been taken, although the analysts may change many of these assumptions, including the level of disaggregation in some, and in particular the base data and behavioral parameters. Like this framework, some economic relationships are chosen a priori, however, and cannot be subsequently changed (without rewriting the model).

APHIS Response: The reviewer is correct, that there are no pre-defined markets and levels of disaggregation built into the modeling framework under discussion, and therefore these definitions are determined as a specific analysis is done. This is in issue of convenience for the analyst rather than an indication of any flaw in the framework itself.

APHIS Response: In the current framework, all of these things need to be done as part of any specific analysis. And they are all different for any specific analysis that might be conducted. It was not the intention within this model, or

it's documentation, to remove these tasks from the analyst. Perhaps, in future development of this framework, some pre-set definitions can be included.

A great advantage of that prior information is that it gives an analyst a starting point. Some data, like base supply utilization balances, are unlikely to need changing. This framework does not provide the analyst a staring point, and so will be less helpful in meeting its "key goal" of "is reducing turnaround time for analyses" (Forsythe, page 4)

A tradeoff in the design decision taken here is that with less information set a priori, less should have to be changed in the process of modeling. But changing parameters in a spreadsheet is a rather trivial task. Finding appropriate parameter assumptions is a much more demanding task, and no help in that direction is provided here. Rewriting cell equations to accommodate new variables would render this approach ineffective.

APHIS Response: The reviewer is correct that finding appropriate parameter assumptions is a much more demanding task than changing the parameters in a spreadsheet. Again, it was not the intention of this model to remove the task of developing parameters from the analyst. It was only to provide a framework within which those parameters could be placed.

While this framework appears to make few theoretical modeling decisions a priori, several are made, and those tend to be buried rather than explicitly discussed. It utilizes basic theory, which is a strength. The analytical structure of the model now brings a degree of inflexibility, however, so that little advantage is gained by having such a structure available. Moreover, the types of shocks to markets modeled here are more easily incorporated in a more direct representation of behavioral equations (they are characterized as quantity shifts here, not price shifts). My guess is that not only would this framework save little time for an APHIS analysts charged with assessing a particular policy, but that analyst would run against constraints to this approach rather quickly, and would want to make different modeling assumptions than those employed here. It would almost always be the case that the derivations described here would need to be redone. That could be avoided by using the solver approach.

In summary, while I do not find fault with the derivations of basic microeconomic theory employed here, I question whether the approach taken to develop this framework will be useful to practical analytical work at APHIS.

APHIS Response: Here is the basic point from the reviewer; that he does not find fault with the derivations of the equations used in the model. Whether the model is of practical value in routine analytical work, versus other possible modeling platforms that might be used, is determined by those who have these various options available to them.

This document explains what was done to develop the spreadsheets that make up this

modeling framework, but not why it was done that way. Avoiding the "why's" leaves unaddressed, and unrecognized, a number of controversies which are implicit in modeling choices now made. The technical strategy for implementing this framework has also fallen behind current spreadsheet capabilities, so that more flexible and complete modeling frameworks are now possible which do not require the tedious derivations presented here.

> APHIS Response: The reviewer's comments are well taken. As mentioned in responses above, the scope of this discussion goes well beyond the intention of the document under consideration. This intention was to describe the mechanics of one possible modeling approach used by APHIS analysts. A broad discussion of all possible modeling approaches could be of great value, including an evaluation of their relative pros and cons.

I doubt that the original goal of this document was achievable. Most analysis of the type AHPHIS must address is not routine. It must be informed by specifics of the problem at hand and by the nature of the narrowly defined markets impacted by the problem. In terms of the work requirements to model such impacts, this framework takes the analyst only a very short distance along the path to informed economic analysis.

> APHIS Response: The reviewer's statement is well taken. It is probably not possible for any modeling framework to do absolutely everything that might be required for any analysis problem that may be conceived. The current modeling framework, however, does incorporate many of the basic methods that have appeared time and time again in APHIS analyses for many years and relieves the analysts of having to re-create this framework each time a new analysis is performed. This one modeling framework is a single tool, among many tools, that can be brought to bear on the variety of analytical problems that APHIS may be faced with. As such, the only intent in its production was to make this framework readily available to analysts as part of the APHIS tool box.

Please let me know if I can be of more help with this review. Best personal regards.

Sincerely,

Philip C. Abbott
Professor

References

Armington, P.S. "A Theory of Demand for Products Distinguished by Place of Production." *International Monetary Fund Staff Papers.* 16(1969a): 159-178.

Bredahl, M.E., W.H. Meyers, and K.J. Collins. "The Elasticity of Foreign Demand for U.S. Agricultural Products: The Importance of the Price Transmission Elasticity." *Amer. J. Agr. Econ.* 61(February 1979):58-63.

Deaton, A. and J. Muellbauer (1980) *Economics and Consumer Behavior*, Cambridge University Press, New York.

Ebel, E.D., R.H. Hornbaker, and C.H. Nelson. "Welfare Effects of the National Pseudorabies Eradication Program." *Amer. J. Agr. Econ.* 74(August 1992):638-45.

Forsythe, K.W., and B.A. Corso. "Welfare Effects of the National Pseudorabies Eradication Program: Comment." *Amer. J. Agr. Econ.* 76(November 1994):968-71.°

Forster and J. Tsai. "A Benefit-Cost Analysis of the National Pseudorabies Program." Final Report from Contract No. 53-6395-2-114, RF Project No. 760341/726861, USDA Animal and Plant Health Inspection Service, Hyattsville, MD, and The Ohio State University, Department of Veterinary Preventive Medicine and Department of Agricultural Economics and Rural Sociology, June 1994

Hertel, T.W. (ed.), *Global Trade Analysis: Modeling and Applications,* Cambridge University Press, 1997.

Hirshleifer, J. 1984. *Price Theory and Applications*, 3rd ed: Prentice-Hall, Inc., Englewood Cliffs, New Jersey.

Hjort, Kim and Pierre Van Peterhem. "The CPPA Model-Builder Technical Structure and Programmed Option in Version 1.3", Agricultural Trade Analysis Division, Economic Research Service, USDA, August 1991.

Lichtenberg, E., D. D. Parker, and D. Zilberman. "Marginal Analysis of Welfare Cost of Environmental Policies: The Case of Pesticide Regulation." *Amer. J. Agr. Econ.* 70(November 1988):867-74.

McCalla, A.F., P. Abbott and P. Paarlberg, "Policy Interdependence, Country Response and the Analytical Challenge," Embargoes, Surplus Disposal, and U.S. Agriculture, Agricultural Economic Report Number 564, Economic Research Service, USDA, Washington, D.C., December 1986.

Paarlberg, P., J. Lee and A. Seitzinger (2003). "Measuring Welfare Impacts of an FMD Outbreak in the United States," *Journal of Agricultural and Applied Economics* 24 (1): 53-65.

Roningen, Vernon O., John Sullivan, and Praveen M. Dixit, Documentation of the Static World Policy Simulation (SWOPSIM) Modeling Framework, Staff Report No.AGES 9151, Economic Research Service, USDA, September 1991.

Van der Mennsbrugghe, D. (2004) 'LINKAGE Technical Reference Document: Version 6.0', mimeo, The World Bank, Washington D.C.